Department of Economics- FEA/USP

Benefit Sharing Exploring Water Resources in Brazil

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Large infrastructure projects may cause permanent impacts on their local environment, affecting the living conditions of local people in a negative manner. To mitigate impacts caused by installation of hydroelectric plants, Brazilian law provides that municipalities are compensated by the *Compensação Financeira pelo Uso de Recursos Hídricos* (CFURH) and the payment of royalties (Itaipu). In this work I test whether such payments have actually worked as a benefit sharing mechanism, examining its effects on some social and economic indicators by way of two steps procedure: first, the propensity score of municipalities benefiting from the compensation is estimated using georeferenced data from WWF's project HydroSHEDS. Then, I estimate a difference-in-differences model, comparing the dependent variables in the control and treatment groups before and after the compensation. The results show a limited effect of the compensation on living conditions. Human Development Index (HDI) and infant mortality showed a little improvement, but illiteracy rate and income inequality worsened.

Keywords: benefit-sharing; CFURH; royalties.

JEL Codes: H75; H76; I38

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Keywords: benefit-sharing, CFURH, royalties, propensity score, diff-in-diff, doubly robust.

JEL Codes: H75, H76, I38

Research area: Applied microeconomics

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1 Introduction

The construction and operation of a hydroelectric power plant may cause major local impacts, both positive (as employment and income growth) and negative (resettlement of families, environmental impacts of flooding large areas, etc.).

In many cases, the negative impacts can be permanent: the impoundment may change the dynamic balance of the aquatic fauna and cause structural changes in vegetation, soil erosion, siltation of rivers, etc. In another dimension, it may be a huge challenge in terms of economic and social development for the localities where the power plant is installed. As Cernea (2008) points out, in many cases the resettlement of families caused by implementing large infrastructure projects has caused impoverishment, largely because the indemnity policies have been insufficient to maintain their living conditions.

To mitigate these adverse effects, Federal Law 7.990/1989 created the *Compensação Financeira Pelo Uso de Recursos Hídricos* (CFURH), a monetary benefit for use of hydraulic potential to electricity generation paid by hydroelectric plants with a nominal capacity of 30 MW to States, Federal District and Municipalities[1].

The main purpose of CFURH is to compensate the municipalities for the loss of area and use of water ANEEL (2014). But more than this, it can be seen as the distribution of economic rents from hydroelectric power generation. According to Rothman (2000), hydroelectric power plants can generate economic rents because there are few places where they can be installed; because some projects can generate power at a lower cost than other technologies or seasonal factors may limit the availability of water in dams. The existence of these revenues is the main reason for adopting mechanisms of benefit sharing with affected municipalities.

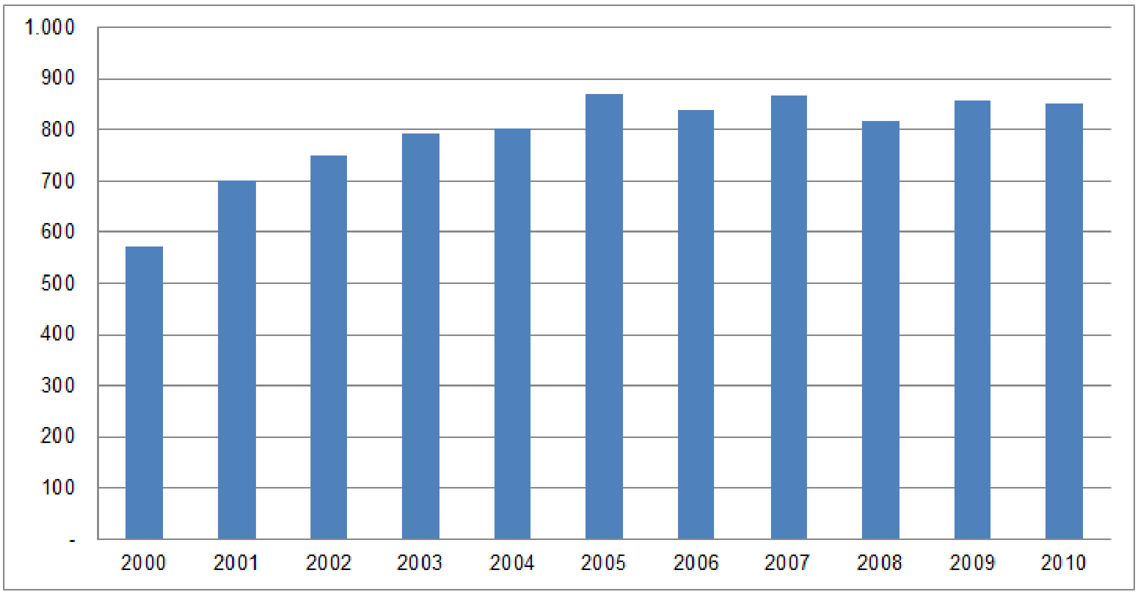
In recent years there has been a considerable increase in the volume of transfers to municipalities by way of compensation. Figure 1 shows the evolution of these transfers, including CFURH and Itaipu royalties. By 2000 the amount of money transfered to affected municipalities was about R$ 570 million while in 2010 it reached around R$ 850 million.

In a scenario of low hydroelectric production, which among other things could be caused by restrictions on the availability of water in reservoirs, the potential loss of this revenue for municipalities could decisively affect their capacity to implement public policies, putting in risk its population living standards. According to ANEEL data, 163 municipalities (nearly a quarter of beneficiaries) received more in compensation than their own tax revenues in 2010, which gives us an idea of how important is this source of funds. Thus, to understand whether these transfers actually had a significant impact on the quality of life for individuals in localities affected by power plants seems to be quite relevant.

The Article 20 of Federal Constitution states that the hydraulic energy potential belongs to the Union and ensures the municipalities, the federal district and states participation in financial compensation for exploiting these resources. Federal Law 7.990/1989 provides that power plants collect monthly to *Secretaria do Tesouro Nacional* (STN) 6.75 % of the value of energy, which is obtained by multiplying the amount of electrical power produced (measured in MW/hour) for *Tarifa Anualizada de Referência* (TAR) calculated

[1] By 1996 the law exempted from contribution plants with nominal capacity up to 10 MW. Federal Law 9,427/1996, which created ANEEL, extended the benefit to enterprises up to 30 MW and self-producers.

Figure 1: **Evolution of CFURH and ITAIPU Royalties distributed to municipalities – 2000-2010 (R$ of 2010)**



Source: Elaborated by the author using data from ANEEL

annually by ANEEL. This same law and its subsequent amendments prohibit the use of these resources to (i) payment of debts (except those contracted by the Union and its institutions) and (ii) permanent staff expenses. [2]

For Itaipu, a bi-national enterprise, compensation takes the form of royalties distributed equally to both countries, as required by Article XV of the Treaty of Itaipu. Royalties calculation is based on the following formula ANEEL (2014):

$$Royalties = \frac{Energy\ Produced\ (GWh) \times US\$650 \times Exchange\ Rate \times 4}{2}$$

Federal Law 9.984/2000 provides that 0.75% of the amount collected should be allocated to the Ministry of Environment to implement the National Water Resources Policy and the National Water Resources Management System. For the remaining, 45% should be distributed to the municipalities and the federal district[3]. The same distribution formula applies to Itaipu royalties. ANEEL inform the STN the amounts to be distributed to municipalities, according to (i) the proportion of flooded area in each municipality and (ii) the energy gain provided to other plants located downstream on the same river.

My goal in this paper is to test the hypothesis that these payments worked as a mechanism for benefit sharing, by examining their impact on a wide range of socioeconomic indicators. For this, I use a two steps procedure: first, I estimated a propensity score of the municipalities affected by CFURH. Second, using the propensity score as weight for the covariates, I estimate a difference-in-differences model, comparing the dependent

[2] Law 12,858/2013 excluded expenses with teachers in public schools from this sealing.
[3] Another 45% goes to states and 10% to the federal government.

variables means in recipient and not recipient municipalities before and after compensation. This method aims to address omitted variable bias, which is the main threat to causal identification of compensation impacts on socioeconomic indicators.

The term benefit sharing is used throughout this paper in the sense defined by Wang (2012): systematic efforts exerted by the proponents of a project to benefit local communities affected by it in a sustainable way. There are some papers dealing with natural resources exploitation that point to the existence of economic rents as main justification for the establishment of benefit sharing mechanisms. Cernea (2008) suggests that these revenues should be used to supplement the indemnities so that localities may invest in the welfare of affected population. He also cites explicitly the case of CFURH in Brazil as an example of benefit sharing which could be incorporated in the guidelines of resettlement policies. McDonald (2006) points out that China was the first country to include in its resettlement policy the notion of opportunities for human development. The author analyzes Three Gorges Dam project, the largest hydroelectric plant in the world, which led to resettlement of more than a million residents. Using surveys and interviews with the affected population he concludes that although there has not been significant improvements in the living conditions of these people, there was neither impoverishment.

However, the empirical evidence is not very clear about the impacts of natural resource rents in promoting welfare. For example, Postali (2009) evaluates the effect of oil royalties on the evolution of local GDP, before and after the passage of *Oil Law* (Federal Law 9,478/1997). He finds that municipalities receiving royalties show lower economic growth than non-beneficiaries, suggesting some kind of resource curse. Similar result was found by Caselli and Michaels (2013), analyzing the economic effects of oil windfalls. They show that royalties windfalls cause a public spending increase in areas such as housing and urban infrastructure, education, health and transport. However, bigger spending does not translate into better social indicators in all these dimensions.

On the other hand, Postali and Nishijima (2013) investigate the impact of oil royalties in several socioeconomic indicators obtained from censuses between 1991 and 2010. The results indicate these revenues contributed to the improvement in urban infrastructure (access to water and electricity) and reduction in the illiteracy rate for beneficiary municipalities.

For the specific case of water resources rents, Monasterio and Sousa (2014) estimate the effects of CFURH on per capita income, tax collection and Human Development Index (HDI), between 2000 and 2010 finding no significant results. According to them, CFURH has none (or even negative) effect on income per capita and HDI growth rates.

One possible explanation for these conflicting results may be sought in the so-called “Natural Resources Curse”. This literature developed by relying, in principle, on the “Dutch Disease” hypothesis: a positive shock on income from natural resources can lead to a reduction in the aggregate income, via appreciation of the exchange rate Sachs and Warner (1995). Over time, new explanations were incorporated, highlighting the role of institutions in the mechanism linking natural resources to economic development. Mehlum et al. (2006) claims that resource abundance can create perverse political incentives that would be mitigated only in countries with good institutions. His main finding is that the quality of institutions determines whether entrepreneurs will specialize in production or rent extraction. Robinson et al. (2006) build a model in which the incumbent politician has to decide how much to extract from the stock of natural resources and how to redistribute the income between his own consumption, transfers to individuals and public sector employment (patronage) so that it can gain political support and be re-elected. This model's main result is that a boom in natural resource rents encourages a redistribution in order to obtain political support in elections, but in countries with good institutions these clientelistic practices may be limited.

An alternative explanation, still related to the contributions mentioned above, can be sought in the political economy literature dealing with how local governments decide to spend resources available in the budget and how voters perceive these expenses. If voters care more about the money coming directly out of their pockets, politicians in office may have less incentive to be diligent with resources transferred by other government levels. Litschig and Morrison (2013) analyze the impact of the *Fundo de Participação dos Municípios* (FPM), a federal transfer for municipalities, on government spending and public goods provision in Brazilian municipalities in the period 1980-1991. They show that increasing transfers via FPM raises education and overall per capita public spending, increases the literacy rate and reduces poverty. In addition, they identify that the increase in public spending increases the probability of re-election for the incumbent party. Brollo et al. (2013) argue that a positive shock in revenues exacerbates the agency problems and deteriorates the pool of candidates running for political office. The incumbent politician faces a trade-off between extracting rents or improving the provision of public goods to please voters and increase his chances of re-election. They test their model's predictions in a sample of Brazilian municipalities, analyzing the impact of FPM in corruption cases detected in audits conducted by the *Controladoria Geral da União* (CGU), an independent branch of federal administration responsible for auditing public spending, and the likelihood of mayoral re-election. The results show that an increase in transfers raises the detection of corruption cases and also the likelihood of re-electing the incumbent, while reducing the proportion of competitors with higher level of education.

In this paper I will investigate whether compensation (CFURH and Itaipu royalties) have affected a number of socioeconomic indicators. The starting point is the already cited paper by Monasterio and Sousa (2014), according to whom "*CFURH revenues are distributed following technical criteria, exogenous to municipalities actions*". In fact, since a hydroelectric plant is installed in a particular locality, it seems reasonable to treat the compensations as exogenous. However, the selection mechanism of the municipality receiving the plant may not be. There are several factors, some observable others not, determining the probability of a given municipality receive a hydroelectric plant. If some of these factors are correlated with socioeconomic indicators, it could confound the causal identification. For instance, the existence of a river with sufficient volume of water is one of the main conditions to install a hydroelectric plant. At the same time, this could also affect the soil agricultural aptitude which can be correlated with the economic growth; or the characteristics of a particular river can determine the presence of certain types of insects, which can be correlated with public health issues, etc.

2 Data

To assess the impact of the compensation policy, I will use the following indicators of the population's living standards: municipal HDI, expected years of schooling, illiteracy rate (population 11-14 years and 15 years or more), infant mortality (deaths per 1,000 live births, up to 1 year old), income per capita (average monthly individual income measured in 2010 R$), Gini index, share of the population living in extreme poverty and the share of population living in households with access to piped water, garbage collection and electricity. All these data were collected in the Atlas of Human Development in Brazil, a database provided by the United Nations Development Programme - UNDP. Because it's census data, they are available only for the years 1991, 2000 and 2010. By 2010, Brazil had 5,565 municipalities, but it was not possible to obtain data for all variables in all periods. Tables 1 and 2 present descriptive statistics and provide a summary of the sample used.

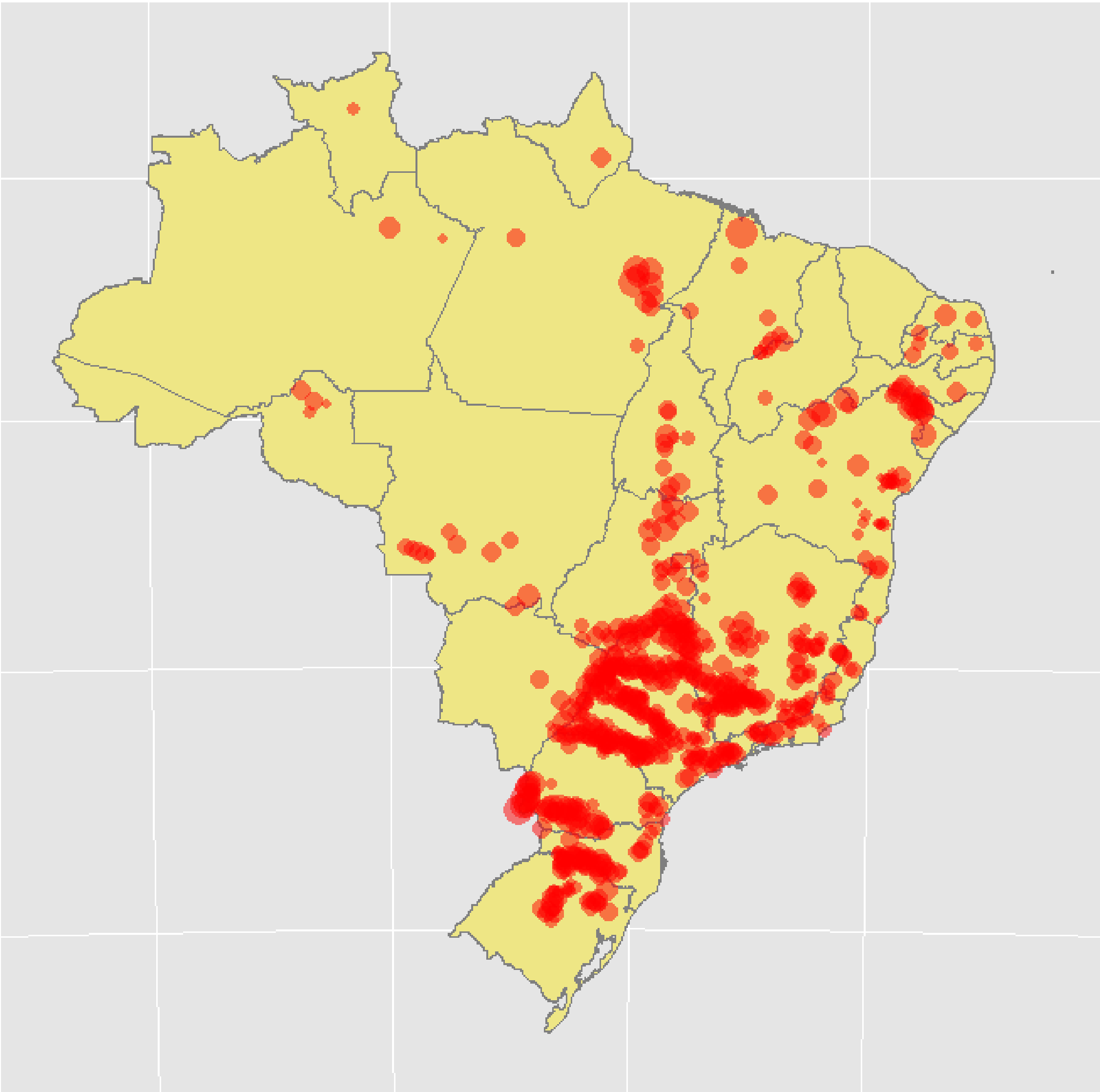
The main concern regarding causal identification resides in the determinants of power plant location, especially the geographic characteristics that determine energy generation potential. The most important of these features is the water flow passing through the turbines to generate power, which basically depends on the amount of water available and variations in elevation of the terrain through which the watercourse runs. In order to control for these characteristics, topography variables were constructed (terrain elevation and water accumulation), inspired by the work of Lipscomb et al. (2013), using the HydroSHEDS database, a WWF (World Wide Fund for Nature) project that uses satellite images to create a georeferenced database with a wide range of information on watersheds. The images have a resolution of 30 arc seconds (each pixel is about 1 km^2 at the equator). Using a GIS (Geographic Information System) tool to open the shapefile of Brazilian municipalities and to overlap it with the satellite images, I calculated the standard deviation of terrain elevation (in meters) and the average of water accumulation (the number of pixels converging) in each pixel. Then, I used the shapefile of Brazilian rivers to create a buffer with 1 km radius around each river, generating 210,809 polygons distributed over Brazilian territory that were superimposed on the municipal grid shapefile. Thus, I was able to extract the averages of water accumulation and variation in terrain elevation within each polygon by municipality, which gives a measure of topographic features in the vicinity of rivers. The reason for creating this measure rather than simply using an average of topographical features within the municipality is to avoid possible biases that could arise if the topographical characteristics were correlated with outcome variables for any other channel besides compensation. For example, if in a particular region within a city there is little variation in terrain elevation, it may be easier (less costly) to build in this site. In such case, estimation may be capturing the effect of economic activity in land occupation, instead of the effect of compensation. By restricting the analysis to characteristics in surrounding rivers, it reduces the possibility that other factors are confounding the identification.

Another important variable to determine the location of a plant is access to power transmission lines. Once again, the shapefile of Brazilian municipalities was used which, when superimposed on the transmission lines shapefile (available in ANEEL website), allowed me to determine all municipalities through which passes some line.

Data on intergovernmental transfers were obtained from the database *Finanças Públics no Brasil* (FINBRA), from STN website, while information about population characteristics were also taken from the Atlas of Human Development in Brazil.

Municipalities benefiting from compensation are distributed in 22 states, but with some concentration in the South and Southeast regions, as shown in Figure 2, reflecting the spatial distribution of economic activities. The size of the circles indicates the amount of compensation per capita.

Figure 2: **Spatial Distribution of Beneficiary Municipalities**



Source: Elaborated by the author using data from ANEEL

3 Empirical Strategy

3.1 Econometric Model

My main interest in this paper is to estimate the impact of compensation on socioeconomic indicators for beneficiary municipalities (treatment group). For this, I would need to know how they would have performed in the case where they did not receive treatment, which can not be observed. However, it is possible to create a control group with similar characteristics to the treated group in all observed dimensions, so that the only difference between the two groups is the existence of treatment.

For this, I use a method in two steps, similar to what has been done by Biondi et al. (2012) and Gadenne (2013). In the first step, using a set X of observed variables, I estimate the probability (propensity score) of a given municipality receive the treatment (compensation), $p(X)$. In the second step, using the propensity score as a weight for each observation, I estimate a difference-in-differences (DID) model comparing the socio-economic indicators in the treatment and control groups, before and after treatment.

The rationale behind this strategy is that by using the propensity score as weight for each observation, greater importance is given to municipalities in the control group that are more similar to those in the treatment group in all observed dimensions, making the two groups comparable.

For the validity of identification two fundamental assumptions must hold:

i. Treatment ignorability: $[Y_1, Y_0 \perp\!\!\!\perp T|X]$, that is, conditional on observed characteristics the potential outcomes are independent of treatment status;

ii. Common Support: $0 < p(T = 1|X) < 1, \forall X$, which means that for all value of X, a given observation has a non-null probability of belonging to either the treatment($T = 1$) or the control($T = 0$) group.

Rosenbaum and Rubin (1983) show that under these two assumptions the so-called strong ignorability is valid:

$$[Y_1, Y_0 \perp\!\!\!\perp T|p(X)] \tag{1}$$

This is an essential hypothesis which greatly simplifies the problem by reducing a covariates vector to a single dimension - the propensity score - making it possible to compare the municipalities in both groups.

As suggested by Dehejia and Wahba (2002), the first step was to estimate a binary response model with a logistic distribution (LOGIT), such that:

$$\hat{p}(X) = \hat{p}(T = 1|X) = f(\beta_0 + \beta_1 X_1 + ... + \beta_k X_k) \tag{2}$$

Where X is a vector k observed variables and $\beta_0, ..., \beta_k$ are the estimated coefficients. Then, the estimated probability is used to weight the vector X, as defined by Imbens and Wooldridge (2009):

$$w_i = \begin{cases} \dfrac{\hat{p}(X_i)}{p} \dfrac{1-p}{1-\hat{p}(X_i)} \dfrac{1-T_i}{1-p} & , \text{ se } T_i = 0 \\ \dfrac{T_i}{p} & , \text{ se } T_i = 1 \end{cases} \tag{3}$$

As $p = \sum_{n=1}^{n_1} \hat{p}(X)|T = 1$ and n_1 the number of units in the treatment group.

Regression of Y_i on X_i weighted by w_i give us the average treatment effect on the treated (ATT)[4].

[4] In the case of the average treatment effect (ATE) weight would be given by:

$$w_i = \begin{cases} \dfrac{1}{1-\hat{p}(X)} & , \text{ se } T_i = 0 \\ \dfrac{1}{\hat{p}(X)} & , \text{ se } T_i = 1 \end{cases}$$

In the second step, I estimate the following model:

$$y_{it} = \delta D_{it} + \beta \tilde{X}_{it} + \lambda_t + \mu_i + \varepsilon_{it} \tag{4}$$

Where y_{it} is the dependent variable in the municipality i and year t, D_{it} is a dummy variable taking the value 1 if municipality i in year t belongs to the treatment group and 0 otherwise, $\tilde{X}$ is the covariates vector weighted by the propensity score varying across municipalities and over time, λ_t is the common effect to all municipalities in the year t and μ_i is a municipality fixed effect. The error term ε_{it} captures unobservable effects that vary over time and between municipalities and it is supposed to be distributed independently of the fixed effects μ_i and λ_t. However, it is possible that ε_{it} are serially correlated between municipalities or time. To deal with this potential problem I calculated the standard errors clustered by state-year.

3.2 Identification Strategy

Here I will discuss the main threats to the identification strategy, which exploits the variation in treatment status across municipalities and over time. The main argument is based on the idea that once we control the factors determining the power plant location, the compensation received by the municipalities can be considered exogenous. This hypothesis seems quite reasonable taking into account that the amounts distributed to municipalities depend on two factors mainly:

(i) The volume of energy produced, which is a technical decision of the plant. A possible argument against would be that entrepreneurs could be concerned with local demand when deciding where to install. While this may have been an issue in the past it is unlikely to currently still influence the decision of the plants. Since mid 1970s the Brazilian electrical system is operated in a coordinated manner through the *Operador Nacional do Sistema* (ONS), a private entity composed by power generation and transmission companies, and regulated by ANEEL. This centralized operation ensures that the energy generated in a given location can be transferred to any other through the transmission network. Thanks to this interconnection, the energy produced in a given municipality will not necessarily meet the demand from this location, removing reverse causality concerns (which could arise if the plant's production decision was influenced by the municipality economic indicators). Currently, less than 2% of the country's energy demand is met out of this system [5].

(ii) The *Tarifa Atualizada de Referência* (TAR), set annually by ANEEL. According to Decree 3.739/2001 the TAR is based on the average prices of electricity sold to the distribution system, excluding the sector charges, taxes, compulsory loans and transmission costs. That is, it is a rule established by a well defined legal framework and not affected by municipalities.

Thus, the major concern regarding identification must be in the selection mechanism of treated municipalities. Two central problems should be considered. The first is reverse causality: the decision of where to locate a plant may be determined by factors specific to the locality, correlated with the outcome variable. For example, if the government chose

[5] Composed primarily of small isolated systems in the Amazon region

a poorer municipality to receive a plant considering stimulate the local economy. But, as said before, the system coordination through the ONS should ease this concern. The second problem is that there may be unobserved factors affecting the treatment group in a different manner that affects the control group. However, Imbens and Wooldridge (2009) show that using propensity score as a weight of the regression attenuates omitted variable bias, leading to an additional robustness in the estimator. According to Bang and Robins (2005), if at least one of the models is correctly specified, the estimator will be consistent (doubly robust).

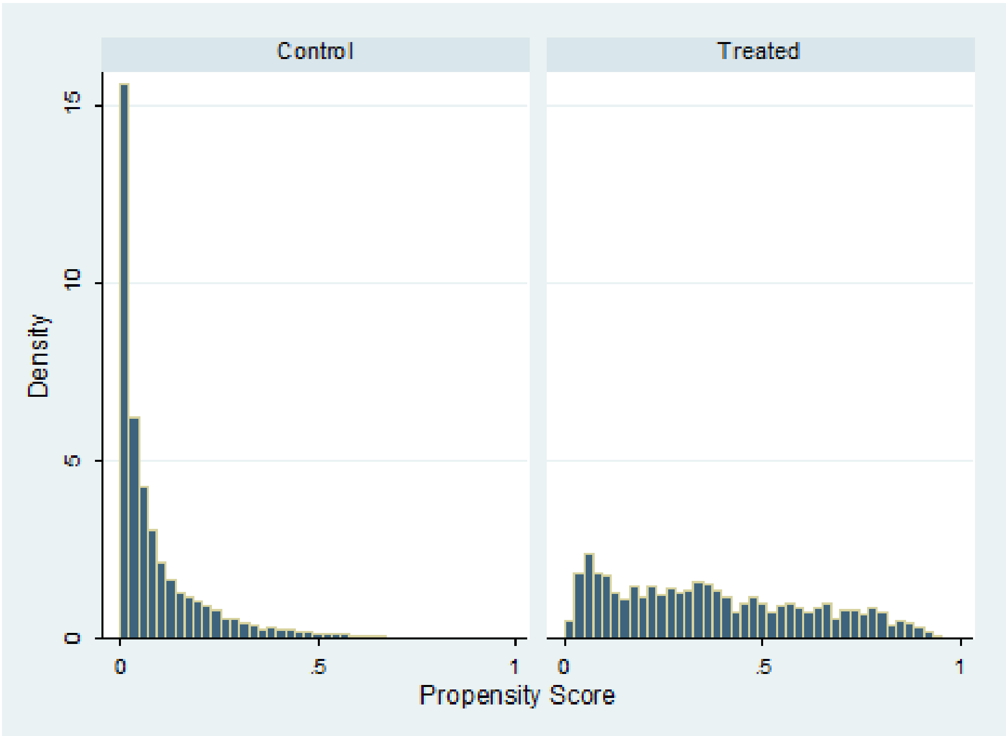
The following variables are included in the vector X:

- Elevation and water accumulation (log): places with higher water accumulation and greater variation in terrain elevation should be more likely to receive a plant;
- Transmission lines (dummy): indicates whether there is a transmission line passing through the municipality. Places with access to transmission lines are more likely to receive a plant;
- Intergovernmental transfers (percent of total revenues): indicates the degree of fiscal fragility of the municipality. The implicit assumption is that municipalities more dependent of other spheres have less discretion over their spending and therefore may have greater difficulty implementing its policies. Alternatively, transfers from other spheres could soften adverse shocks in local economic activity;
- GDP per capita (R$): controls for the wealth in the municipality. The assumption is that richer municipalities should have better indicators;
- Population (log): Controls for the size of the municipality. The hypothesis is that more populous municipalities require more infrastructure. On the other hand, it allows larger economies of scale on the adopted policies;
- Area (km^2): another control to municipality size. The assumption is that larger municipalities also require more infrastructure, but may have greater economic potential;
- Dependency ratio (ratio between the number of people aged under 14 and over 65 on the population aged between 15 and 64): Hypothesis that municipalities with the highest dependency ratio should present worse indicators, since these municipalities need increase spending on health and education;
- Urban population (percent of total population): Controls the degree of urbanization. More urbanized municipalities tend to have better infrastructure and better socioeconomic indicators;
- Dummies for region (NO, NE, S, SE, CO): capture fixed effects of geographic region.

4 Results

The first step is to estimate the LOGIT model obtaining the propensity score. As can be seen in Figure 3 there is a large mass of municipalities in the control group concentrated around zero, suggesting that the naive estimation of a fixed effects panel could be biased, because we would be comparing two groups quite different. Table 4 shows the first step results. Including all the covariates presented before, the pseudo R^2 was 0.296. Most of the estimates were significant at the 1% level, except for the variable GDP per capita which is significant only at the 5% level.

Figure 3: **Propensity Score Distribution, by group**



In the first step, the goal is to create a common support for the observations, as a function of the vector of observed variables, increasing the comparability of treatment and control groups. In other words, we need to ensure that every observation in the treatment group can be compared to at least one observation in the control group. Here, I followed the procedure adopted by Galiani et al. (2005):

a) In the lower limit were excluded observations with propensity score below the first percentile in the treatment group;

b) In the upper limit were excluded observations with propensity score above the 99th percentile in the control group.

Once the common support was created, we need to ensure that treatment and control groups are in fact comparable, i.e., whether the covariates are balanced in order to avoid selection bias. Table 5 shows that balancing seems to have been successful. The first column presents differences in means between treated and untreated municipalities, without any control which is our baseline. In the second column, we have the differences only for municipalities within the common support. Finally, the third column shows the difference in means for municipalities belonging to the common support and conditional to the propensity score. In such specification only the variable Elevation has a difference statistically significant at the 10% level, but such difference does not reach 1% of the mean for that variable.

To ensure that these groups were in fact well balanced, I tested the difference in means for every decile of each variable's distribution. The results are reported in Table 3. Although almost all the variables present any statistically significant difference in some decile, these are of small magnitude. In all cases, the difference never reaches 10% of the respective variable mean, suggesting that treatment and control groups show good comparability.

4.1 General Results

The main interest in this paper is to estimate the average treatment effect on the treated (ATT). Second step estimation, i.e., equation (4) is detailed in Tables 8 and 9. Results show that compensations have a slightly positive[6] impact on HDI and Infant Mortality (equivalent to approximately 2.5% from their respective means for the treatment group in 1991); a bit higher, but negative on Illiteracy 11-14 years and a small negative[7] impact for Gini Index (equivalent to about 10% and 2% of the respective means for treatment group in 1991). For other indicators, no statistically significant effects were found.

As covariates are weighted by propensity score, their estimates can not be interpreted as marginal effects. In such case, the most suitable would be analyze the sign of these correlations. As already pointed out, there are two variables for which it is expected a clearer direction: GDP per Capita and Urban Population. In the case of Urban Population, the results go in the expected direction, showing that more urbanized municipalities have better indicators. In the case of variable GDP per capita, the correlation with Infant Mortality has a sign opposite to what would be expected, but this is only significant at the 10% level.

One possible concern is that the errors show some kind of serial correlation, either between municipalities or over time. To overcome this problem, the model was estimated again, clustering the standard errors at state-year level. These results are shown in Table 10. Column (1) shows the same results of Tables 8 and 9 as baseline while in column (2) we have the results from the new estimation. The effects remain significant for HDI and Gini Index, but lose significance in Illiteracy 11-14 years and Infant Mortality, suggesting that some kind of serial correlation could exist.

4.2 Heterogeneous Effects

This section reviews the possible heterogeneous effects. It seems reasonable to assume that not all municipalities will be affected the same way. The first form of heterogeneity explored is the amount of funds distributed to municipalities. In order to assess these effects, I estimate equation 4 again by restricting the sample to municipalities in which the amount of compensation per capita overcomes certain thresholds. One would expect that municipalities receiving more resources to fund more and better policies, thus achieving stronger results. The results of Table 11 show that this happens in a clearer way for the Infant Mortality dimension. The point estimates of the variables Years of Schooling, Illiteracy 11-14 years and Illiteracy 15+ years suggest the same, but they are

[6] The negative signal in Infant Mortality means that the treated municipalities had a more marked reduction in this indicator than the untreated municipalities, hence why it is considered a positive impact

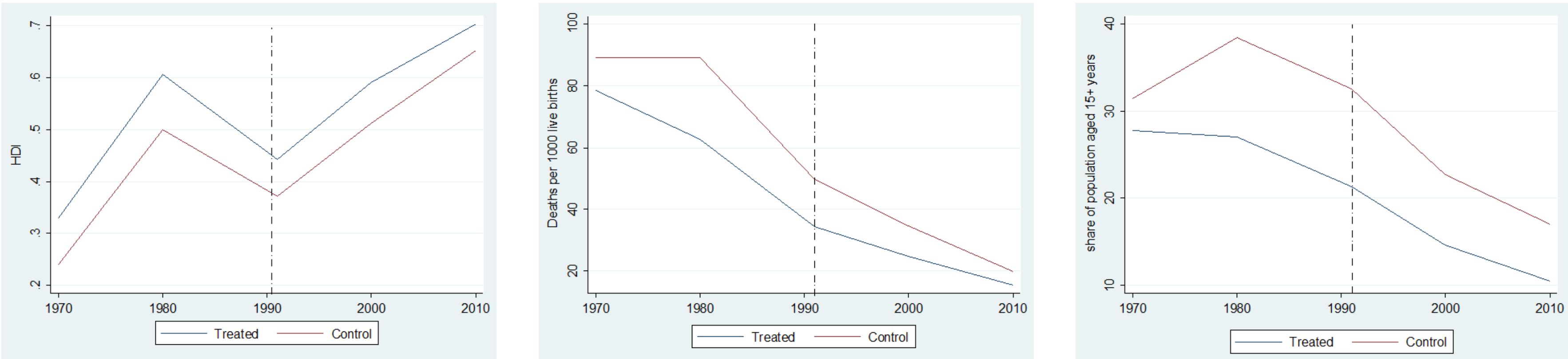
[7] Likewise, a positive result suggests that these indicators developments in municipalities treated were lower than in untreated ones, hence the negative impact.

not statistically significant. The variables Illiteracy 11-14 years and Illiteracy 15+ years are statistically significance at the 10% level only when the sample is restricted to municipalities receiving compensation above R$ 40 per capita. All those results must be viewed with some caution, because there is a considerable loss in the number of observations when the sample is restricted.

The second form of heterogeneity it refers to time of exposure to treatment. For this, I restricted the sample of treated municipalities to those who began to receive compensation before the year 2000. It would be expected that these municipalities make better indicators, since it could take some time so that they can create more structured policies. Table 12 seems to confirm this assumption. The first column includes all the municipalities that received some compensation, since it has been before 2000. These estimates are directly comparable to the column (2) of Table 10. The other columns are comparable to Table 11. The results show that almost all indicators are better when municipalities receive compensation for longer and are even better when these same municipalities receive more resources.

5 Robustness Check

The main threat to the adopted identification strategy is the existence of unobservable characteristics that can be correlated with either the selection mechanism for treatment as to the outcome variables. Estimating a difference-in-differences model solve part of this problem, because it compares the variation in mean of the outcome variables for treated municipalities with the variation in mean for untreated municipalities, thus controlling for time-invariant characteristics. If treatment and control groups are sufficiently similar, the variation in mean for the control group is a good estimate for the counterfactual, i.e., what would have happened with the treated municipalities if they had not received treatment. The most important identifying hypothesis in this framework is that treated and untreated municipalities would present the same behavior if there were no compensation, but this assumption is not testable. What we can do is check whether the two groups showed a common trend in dependent variables before the existence of treatment. Unfortunately it was not possible to obtain data prior to 1991 for some of the dependent variables. Figure 4 shows the evolution of HDI, Infant Mortality and Illiteracy 15+ years for the period 1970-2010.



(a) HDI (b) Infant Mortality (c) Illiteracy 15+ years

Figure 4: **Evolution of socioeconomic indicators for treated and untreated municipalities: 1970-2010**

As can be seen, the trend for treated municipalities is similar to that for untreated ones in the decade prior to implementing compensation. Coupled with the fact that balancing of covariates appears to have been successful, this suggests that if there are any time-varying unobservable factor affecting both the treatment and outcome variables, it affects both groups the same way and, therefore, should not be confounding identification.

Another concern raised is that some policy held by other government level may be affecting the outcome variables. For example, some municipalities could have some agreement with the state government or perhaps could be using resources of regional development funds to finance a specific program. Trying to overcome this problem, I estimated model (4) again including specific trends for state and geographic region. Table 13 shows that the inclusion of this control have little impact on the results. Only the coefficient for Illiteracy 11-14 years slightly increases and becomes significant at the 5% level, suggesting that there may be some policy implemented in another sphere of government that mitigates the negative impact of compensation in this dimension.

Finally, I consider the hypothesis that the very establishment of the power plants may have somehow affected the development of the municipalities where they were installed. This means that the effect captured in the estimation would not be the true effect of the treatment (to receive compensation), because it was being confounded with a long-term effect induced by the construction of the plants. To deal with this problem I estimated again both the first and second steps, excluding from the sample municipalities in which dams or engine rooms were built[8]. Thus, municipalities that remain in the treatment group are only those who are being affected by the policy via compensation. Table 14 repeats the previous table applying this restriction to the sample. The results show there is little difference between the two scenarios, suggesting that the construction of the plants did not generate very significant long term impacts on socioeconomic indicators for the municipalities that received them.

6 Concluding Remarks

The results found in this study suggest that the impact of CFURH and Itaipu royalties on socioeconomic indicators for beneficiary municipalities was rather limited. Although the HDI and Infant Mortality have shown a slight improvement, Illiteracy rate for the population between 11 and 14 years old and income inequality (measured by the Gini Index) worsened. However, these effects are not the same for all treated municipalities. Those that receive higher compensation, for longer, have a much better performance than others in virtually all dimensions evaluated, suggesting that the mechanism of benefit sharing here is longer term.

The empirical strategy used took into account the main threat to causal identification of effects: the omitted variable bias. The results appear robust to possible problems in the specification.

One possible implication in terms of public policy is put into debate the distribution criteria of compensation between the federal entities. It is curious that the states fit the same amount of resources that is intended for municipalities, since the latter are the largest affected by the power plants.

[8] To ensure the validity of the identification hypothesis, balancing of covariates was tested in this new sample, both in average and by decile. Again, the results reported in Tables 15 and 16 suggest that the balancing was successful.

The mechanism through which these impacts occur can be the subject of future research.

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Table 1: Descriptive Statistics : Dependent Variables

	1991		2000		2010	
	Treated	Control	Treated	Control	Treated	Control
HDI	0.458	0.395	0.592	0.517	0.702	0.651
	(.075)	(.095)	(.078)	(.102)	(.0541)	(.0712)
Years of Schooling	8.845	7.666	9.258	8.261	9.786	9.388
	(1.362)	(1.911)	(1.396)	(1.752)	(1.019)	(1.079)
Illiteracy 11-14 years	8.435	20.863	3.213	7.883	2.008	4.006
	(10.197)	(18.732)	(4.339)	(8.263)	(1.998)	(3.879)
Illiteracy 15+ years	20.408	31.291	14.623	22.600	10.612	17.241
	(9.431)	(16.609)	(7.202)	(12.614)	(5.762)	(10.019)
Infant Mortality	33.246	48.655	24.764	34.220	15.480	19.942
	(13.127)	(23.316)	(8.312)	(14.002)	(4.256)	(7.283)
Income per capita	337.467	240.066	454.055	325.846	624.528	466.399
	(142.235)	(141.205)	(177.665)	(186.989)	(206.477)	(235.481)
Extreme Poverty	15.556	30.362	9.797	21.686	4.886	12.511
	(13.799)	(20.214)	(10.581)	(17.083)	(6.834)	(11.977)
Gini Index	0.536	0.533	0.541	0.549	0.481	0.498
	(.062)	(.065)	(.060)	(.068)	(.059)	(.065)
Piped Water	77.703	56.867	85.085	65.511	92.323	84.442
	(19.494)	(29.581)	(19.100)	(28.588)	(8.795)	(15.196)
Garbage Collection	75.276	60.692	91.304	80.265	97.790	93.534
	(22.820)	(28.092)	(14.125)	(22.649)	(4.814)	(11.205)
Access to Electricity	87.721	73.649	93.956	86.317	98.624	96.952
	(14.697)	(22.284)	(10.882)	(16.752)	(3.832)	(6.238)
Municipalities	568	3,132	693	4,451	692	4,618

Notes: Standard deviations in parenthesis.

Table 2: Descriptive Statistics: Covariates

	1991		2000		2010	
	Treated	Control	Treated	Control	Treated	Control
Elevation	4.184	4.11	4.182	4.064	4.181	4.04
	(.625)	(.748)	(.633)	(.768)	(.629)	(.781)
Water Accumulation	9.230	7.476	9.311	7.542	9.291	7.579
	(1.760)	(1.602)	(1.756)	(1.672)	(1.755)	(1.693)
Transmission Lines	0.627	0.426	0.631	0.405	0.633	0.403
	(.484)	(.495)	(.483)	(.491)	(.482)	(.490)
Intergovernmental Transfers	74.828	75.885	80.239	85.587	87.785	92.408
	(14.182)	(15.299)	(12.140)	(11.323)	(13.418)	(12.639)
GDP per capita	6.185	4.388	5.967	3.826	8.016	5.295
	(9.221)	(11.102)	(6.187)	(4.317)	(6.403)	(6.148)
Population	9.553	9.512	9.495	9.374	9.575	9.442
	(1.135)	(1.003)	(1.207)	(1.095)	(1.240)	(1.121)
Area	6.438	6.241	6.381	6.171	6.392	6.219
	(1.115)	(1.255)	(1.121)	(1.273)	(1.122)	(1.287)
Dependency Ratio	64.328	74.519	55.865	62.881	47.558	52.219
	(10.605)	(15.745)	(8.422)	(12.057)	(6.232)	(9.029)
Urban Population	66.216	55.783	69.416	58.129	73.995	62.525
	(20.755)	(21.994)	(21.559)	(23.041)	(19.961)	(21.860)
Municipalities	568	3,132	693	4,451	692	4,618

Notes: Standard deviations in parenthesis.

Table 3: Balancing of covariates, by decile

	Elevation	Water Accum.	Intergov. Transfers	GDP per capita	Population	Area	Dependency Ratio	Urban Population
1st Decile	0.350***	0.030	0.819	-0.084**	-0.008	0.102*	0.332*	-0.214
	(0.087)	(0.061)	(0.825)	(0.035)	(0.025)	(0.054)	(0.172)	(0.680)
2nd Decile	0.015	-0.008	-0.106	0.009	0.007	-0.004	-0.258***	0.103
	(0.009)	(0.017)	(0.211)	(0.018)	(0.011)	(0.012)	(0.086)	(0.322)
3rd Decile	-0.008	-0.009	-0.023	0.022	0.000	-0.002	-0.070	0.032
	(0.007)	(0.016)	(0.142)	(0.017)	(0.009)	(0.008)	(0.080)	(0.285)
4th Decile	-0.007	0.001	0.005	0.009	-0.005	0.001	-0.044	0.061
	(0.006)	(0.014)	(0.110)	(0.020)	(0.008)	(0.007)	(0.078)	(0.224)
5th Decile	-0.003	-0.059***	0.001	0.021	0.021**	-0.000	0.046	0.076
	(0.006)	(0.015)	(0.095)	(0.021)	(0.009)	(0.007)	(0.081)	(0.226)
6th Decile	0.006	-0.005	0.198**	-0.003	0.003	0.003	0.121	-0.051
	(0.004)	(0.015)	(0.089)	(0.020)	(0.007)	(0.009)	(0.094)	(0.197)
7th Decile	-0.013***	-0.004	0.038	-0.006	0.004	0.034***	0.071	0.289
	(0.005)	(0.014)	(0.091)	(0.020)	(0.008)	(0.009)	(0.160)	(0.199)
8th Decile	-0.010*	0.000	-0.006	0.007	-0.002	-0.017	-0.372	0.188
	(0.005)	(0.016)	(0.084)	(0.027)	(0.009)	(0.013)	(0.256)	(0.185)
9th Decile	-0.003	-0.000	-0.357***	0.015	0.028*	-0.020	1.376***	-0.096
	(0.007)	(0.020)	(0.120)	(0.051)	(0.015)	(0.021)	(0.484)	(0.185)
10th Decile	-0.007	-0.418***	-0.122	0.638	0.073	-0.144**	-0.857	0.071
	(0.022)	(0.061)	(0.391)	(0.994)	(0.061)	(0.069)	(1.284)	(0.228)

Notes: All columns show the difference in means between treatment and control groups, by decile of the respective variable. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.

Table 4: First Step Estimation - Propensity Score

Elevation	0.402***	Area	0.240***
	(0.044)		(0.033)
Water Accumulation	0.696***	Urban Population	0.008***
	(0.019)		(0.002)
Transmission Lines	0.609***	South Dummy	0.555***
	(0.059)		(0.125)
Intergovernmental Transfers	-0.012***	Dummy Região Sudeste	1.424***
	(0.002)		(0.110)
Population	-0.198***	North Dummy	-0.885***
	(0.039)		(0.223)
GDP per capita	0.008**	Northeast Dummy	-0.392***
	(0.004)		(0.146)
Dependency Ratio	-0.045***	Year = 2000	-0.439***
	(0.004)		(0.082)
Constant	-6.478***	Year = 2010	-0.827***
	(0.541)		(0.108)
Observations	14.676		
Pseudo R^2	0,296		

Notes: The dependent variable is a dummy which takes the value 1 if the municipality belongs to the treatment group, 0 otherwise. Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 5: Covariates Balancing

	Baseline	Common Support	Common Support & Propensity Score
Elevation	0.146***	0.049**	0.040*
	(0.018)	(0.020)	(0.021)
Water Accumulation	1.727***	0.905***	0.016
	(0.039)	(0.047)	(0.044)
Transmission Lines	0.226***	0.118***	0.002
	(0.011)	(0.014)	(0.014)
Intergovernmental Transfers	-3.878***	-1.379***	0.229
	(0.349)	(0.396)	(0.421)
GDP per Capita	2.304***	0.975***	0.300
	(0.172)	(0.177)	(0.189)
Population	0.131***	0.140***	0.029
	(0.026)	(0.032)	(0.034)
Area	0.175***	0.074**	0.014
	(0.030)	(0.036)	(0.038)
Dependency Ratio	-7.666***	-1.309***	0.366
	(0.371)	(0.350)	(0.371)
Urban Population	0.110***	0.047***	-0.004
	(0.005)	(0.006)	(0.006)

Notes: Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 6: Second Step Estimation - Average Treatment Effect on the Treated (ATT)

	HDI	Years of Schooling	Income per Capita	Piped Water	Garbage Collection	Access to Electricity
ATT	0.011***	-0.099	5.703	1.631	0.576	0.482
	(0.003)	(0.082)	(9.927)	(1.183)	(1.196)	(0.718)
Intergov. Transfers	-0.121*	3.437*	-499.994***	50.189**	80.564**	24.802
	(0.065)	(2.026)	(178.469)	(25.173)	(33.506)	(17.675)
Dependency Ratio	-1.659***	-54.066***	3,904.415***	-975.786***	-857.415***	-680.036***
	(0.138)	(5.328)	(428.705)	(76.007)	(99.683)	(68.200)
GP per Capita	0.184	-0.112		22.118	8.774	9.253
	(0.112)	(2.738)		(38.430)	(71.317)	(27.325)
Population	-18.078***	-216.427	-5,750.366	-6,666.319***	-10,518.721***	-8,573.372***
	(5.391)	(153.809)	(21,939.261)	(2,296.933)	(2,888.453)	(1,906.714)
Area	-1.905	263.513**	-21,182.358	-5,312.863***	-3,601.908	-3,537.328**
	(4.164)	(119.301)	(13,269.100)	(1,571.139)	(2,309.758)	(1,630.101)
Urban Population	0.928***	3.997	1,232.224***	246.607***	33.775	246.336***
	(0.147)	(3.544)	(416.506)	(60.797)	(69.336)	(42.903)
Year = 2000	0.106***	-0.153	168.822***	-6.636***	6.476***	-2.330**
	(0.004)	(0.108)	(12.090)	(1.633)	(1.919)	(1.129)
Year = 2010	0.195***	-0.188	384.294***	-11.685***	3.671	-6.161***
	(0.006)	(0.166)	(17.332)	(2.404)	(2.959)	(1.730)
Constant	0.769***	13.260***	173.919	264.282***	300.359***	260.279***
	(0.086)	(2.438)	(321.107)	(35.155)	(43.380)	(30.500)
Observations	1,543	1,543	1,543	1,543	1,535	1,543
Municipalities	587	587	587	587	587	587

Notes: All the covariates are weighted by the propensity score. The covariates Elevation, Water Accumulation and Transmission Lines are time-invariant, so their effect disappears in the DID model. In this specification positive values of ATT show improvement in the socioeconomic indicators. Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 7: Second Step Estimation - Average Treatment Effect on the Treated (ATT)

	Illiteracy 11-14 years	Illiteracy 15+ years	Infant Mortality	Extreme Poverty	Gini Index
ATT	0.839**	0.080	-0.850*	-0.015	0.010*
	(0.385)	(0.181)	(0.467)	(0.482)	(0.005)
Intergov. Transfers	-73.054***	-29.863***	-88.180***	-30.059**	0.108
	(12.944)	(6.612)	(17.483)	(12.745)	(0.116)
Dependency Ratio	609.790***	301.928***	612.922***	431.074***	-1.312***
	(39.816)	(19.885)	(52.961)	(36.473)	(0.313)
GDP per Capita	-3.316	-1.892	51.485*	18.828	0.234
	(15.269)	(7.514)	(28.964)	(19.367)	(0.201)
Population	1,955.044*	803.697	3,606.305**	13,921.561***	8.608
	(1,095.627)	(631.934)	(1,465.040)	(1,164.463)	(9.825)
Area	-365.443	-644.677	-969.404	1,527.776	-7.142
	(651.767)	(433.748)	(1,283.694)	(1,272.614)	(8.418)
Urban Population	-36.535	-44.954***	-7.362	-95.301***	-0.185
	(22.935)	(15.946)	(34.520)	(27.345)	(0.249)
Year = 2000	1.556***	-2.255***	-0.727	-2.493***	-0.023***
	(0.577)	(0.305)	(0.818)	(0.763)	(0.007)
Year = 2010	7.890***	-2.431***	-2.897**	-3.216***	-0.102***
	(0.973)	(0.535)	(1.423)	(1.175)	(0.010)
Constant	-55.380***	-3.089	-47.216*	-197.517***	0.611***
	(16.452)	(9.584)	(24.243)	(19.431)	(0.167)
Observations	1,521	1,543	1,543	1,533	1,543
Municipalities	583	587	587	586	587

Notes: All the covariates are weighted by the propensity score. The covariates Elevation, Water Accumulation and Transmission Lines are time-invariant, so their effect disappears in the DID model. For this specification negative values of ATT show improvement in the socioeconomic indicators. Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 8: Second Step Estimation - Average Treatment Effect on the Treated (ATT)

	HDI	Years of Schooling	Income per Capita	Piped Water	Garbage Collection	Access to Electricity
ATT	0.011***	-0.099	5.703	1.631	0.576	0.482
	(0.003)	(0.082)	(9.927)	(1.183)	(1.196)	(0.718)
Intergov. Transfers	-0.121*	3.437*	-499.994***	50.189**	80.564**	24.802
	(0.065)	(2.026)	(178.469)	(25.173)	(33.506)	(17.675)
Dependency Ratio	-1.659***	-54.066***	3,904.415***	-975.786***	-857.415***	-680.036***
	(0.138)	(5.328)	(428.705)	(76.007)	(99.683)	(68.200)
GP per Capita	0.184	-0.112		22.118	8.774	9.253
	(0.112)	(2.738)		(38.430)	(71.317)	(27.325)
Population	-18.078***	-216.427	-5,750.366	-6,666.319***	-10,518.721***	-8,573.372***
	(5.391)	(153.809)	(21,939.261)	(2,296.933)	(2,888.453)	(1,906.714)
Area	-1.905	263.513**	-21,182.358	-5,312.863***	-3,601.908	-3,537.328**
	(4.164)	(119.301)	(13,269.100)	(1,571.139)	(2,309.758)	(1,630.101)
Urban Population	0.928***	3.997	1,232.224***	246.607***	33.775	246.336***
	(0.147)	(3.544)	(416.506)	(60.797)	(69.336)	(42.903)
Year = 2000	0.106***	-0.153	168.822***	-6.636***	6.476***	-2.330**
	(0.004)	(0.108)	(12.090)	(1.633)	(1.919)	(1.129)
Year = 2010	0.195***	-0.188	384.294***	-11.685***	3.671	-6.161***
	(0.006)	(0.166)	(17.332)	(2.404)	(2.959)	(1.730)
Constant	0.769***	13.260***	173.919	264.282***	300.359***	260.279***
	(0.086)	(2.438)	(321.107)	(35.155)	(43.380)	(30.500)
Observations	1,543	1,543	1,543	1,543	1,535	1,543
Municipalities	587	587	587	587	587	587

Notes: All the covariates are weighted by the propensity score. The covariates Elevation, Water Accumulation and Transmission Lines are time-invariant, so their effect disappears in the DID model. In this specification positive values of ATT show improvement in the socioeconomic indicators. Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 9: Second Step Estimation - Average Treatment Effect on the Treated (ATT)

	Illiteracy 11-14 years	Illiteracy 15+ years	Infant Mortality	Extreme Poverty	Gini Index
ATT	0.839**	0.080	-0.850*	-0.015	0.010*
	(0.385)	(0.181)	(0.467)	(0.482)	(0.005)
Intergov. Transfers	-73.054***	-29.863***	-88.180***	-30.059**	0.108
	(12.944)	(6.612)	(17.483)	(12.745)	(0.116)
Dependency Ratio	609.790***	301.928***	612.922***	431.074***	-1.312***
	(39.816)	(19.885)	(52.961)	(36.473)	(0.313)
GDP per Capita	-3.316	-1.892	51.485*	18.828	0.234
	(15.269)	(7.514)	(28.964)	(19.367)	(0.201)
Population	1,955.044*	803.697	3,606.305**	13,921.561***	8.608
	(1,095.627)	(631.934)	(1,465.040)	(1,164.463)	(9.825)
Area	-365.443	-644.677	-969.404	1,527.776	-7.142
	(651.767)	(433.748)	(1,283.694)	(1,272.614)	(8.418)
Urban Population	-36.535	-44.954***	-7.362	-95.301***	-0.185
	(22.935)	(15.946)	(34.520)	(27.345)	(0.249)
Year = 2000	1.556***	-2.255***	-0.727	-2.493***	-0.023***
	(0.577)	(0.305)	(0.818)	(0.763)	(0.007)
Year = 2010	7.890***	-2.431***	-2.897**	-3.216***	-0.102***
	(0.973)	(0.535)	(1.423)	(1.175)	(0.010)
Constant	-55.380***	-3.089	-47.216*	-197.517***	0.611***
	(16.452)	(9.584)	(24.243)	(19.431)	(0.167)
Observations	1,521	1,543	1,543	1,533	1,543
Municipalities	583	587	587	586	587

Notes: All the covariates are weighted by the propensity score. The covariates Elevation, Water Accumulation and Transmission Lines are time-invariant, so their effect disappears in the DID model. For this specification negative values of ATT show improvement in the socioeconomic indicators. Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 10: Average Treatment Effect on the Treated (ATT), adjusting for serial correlation

	(1)	(2)
HDI	0.011***	0.011***
	(0.003)	(0.004)
Years of Schooling	-0.099	-0.099
	(0.082)	(0.106)
Income per capita	5.703	5.703
	(9.927)	(12.214)
Piped Water	1.631	1.631
	(1.183)	(2.209)
Garbage Collection	0.576	0.576
	(1.196)	(2.004)
Acess to Electricity	0.482	0.482
	(0.718)	(1.239)
Illiteracy 11-14 years	0.839**	0.839
	(0.385)	(0.528)
Illiteracy 15+ years	0.080	0.080
	(0.181)	(0.274)
Infant Mortality	-0.850*	-0.850
	(0.467)	(0.872)
Extreme Poverty	-0.015	-0.015
	(0.482)	(0.722)
Gini Index	0.010**	0.010**
	(0.005)	(0.005)
Observations	1,521	1,521
Municipalities	583	583
Clusters	-	61

Notes: For column 1, robust standard errors in parenthesis. In column 2, standard errors clustered at state-year level. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 11: Heterogeneous Effects

	Compensation (R$ per capita)					
	≥ 5	≥ 10	≥ 20	≥ 30	≥ 40	≥ 50
HDI	0.008**	0.006*	0.005	0.004	0.004	0.004
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Years of Schooling	-0.128	-0.120	-0.068	-0.061	-0.010	0.025
	(0.090)	(0.089)	(0.097)	(0.119)	(0.129)	(0.154)
Income per capita	-10.860	-15.813	-12.111	-16.777	-13.989	-15.663
	(13.729)	(14.146)	(12.755)	(12.928)	(14.247)	(16.545)
Piped Water	1.407	0.806	1.859	2.874	2.430	2.433
	(2.245)	(2.118)	(2.112)	(2.203)	(2.422)	(2.846)
Garbage Collection	3.215	3.313	3.213	3.787*	3.111	3.777
	(2.127)	(2.075)	(2.128)	(2.230)	(2.427)	(2.582)
Access to Electricity	1.182	0.911	0.616	1.005	0.595	0.537
	(1.196)	(1.220)	(1.158)	(1.208)	(1.307)	(1.523)
Illiteracy 11-14 years	-0.252	-0.648	-0.709	-1.050	-1.645*	-1.800*
	(0.774)	(0.815)	(0.760)	(0.821)	(0.852)	(0.930)
Illiteracy 15+ years	-0.407	-0.379	-0.464	-0.426	-0.617*	-0.683*
	(0.353)	(0.361)	(0.328)	(0.341)	(0.361)	(0.393)
Infant Mortality	-1.470	-2.005*	-2.134*	-2.363*	-2.872**	-2.643*
	(0.936)	(1.073)	(1.153)	(1.263)	(1.349)	(1.504)
Extreme Poverty	0.272	0.495	0.807	0.324	0.125	0.783
	(0.648)	(0.706)	(0.695)	(0.723)	(0.728)	(0.789)
Gini Index	0.018***	0.021***	0.020***	0.018***	0.021***	0.018**
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)
Fixed Effects	Year, Mun	Year, Mun	Year, Mun	Year, Mun	Year, Mun	Year, Mun
Observations	1,127	1,010	922	853	806	766
Municipalities	567	558	551	542	536	531
Clusters	131	130	120	115	108	105

Notes: Standard errors clustered at state-year level in parenthesis. *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table 12: Heterogeneous Effects: Municipalities where compensation started before 2000

	Compensation (R$ per capita)						
	≥ 0	≥ 5	≥ 10	≥ 20	≥ 30	≥ 40	≥ 50
HDI	0.068***	0.070***	0.074***	0.079***	0.082***	0.078***	0.080***
	(0.012)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)
Years of Schooling	-0.178	-0.287**	-0.071	-0.013	-0.021	-0.082	-0.204
	(0.125)	(0.132)	(0.132)	(0.134)	(0.146)	(0.163)	(0.159)
Income per capita	46.332	38.175	50.980*	75.619**	81.502**	96.918***	106.455***
	(29.223)	(26.600)	(29.321)	(30.006)	(33.814)	(33.659)	(33.711)
Piped Water	-1.554	-0.836	-0.390	2.847	2.938	1.407	-0.009
	(3.289)	(4.154)	(4.039)	(3.699)	(3.891)	(4.097)	(4.956)
Garbage Collection	6.431*	9.279**	11.576**	11.508**	10.728**	7.813	8.456
	(3.822)	(4.618)	(4.907)	(5.123)	(5.072)	(5.059)	(5.149)
Access to Electricity	-0.209	0.217	0.830	1.101	0.431	-0.787	-0.898
	(2.104)	(2.129)	(1.802)	(2.115)	(2.196)	(2.135)	(2.258)
Illiteracy 11-14 years	-0.523	-1.213	-0.822	-0.520	-0.149	-0.041	0.073
	(1.328)	(1.348)	(1.439)	(1.611)	(1.798)	(1.866)	(1.542)
Illiteracy 15+ years	-2.180***	-2.620***	-2.456***	-2.812***	-2.859***	-2.899***	-3.191***
	(0.265)	(0.424)	(0.471)	(0.458)	(0.599)	(0.609)	(0.631)
Infant Mortality	-0.282	-0.532	-1.201	-1.795	-2.248	-2.115	-2.036
	(1.027)	(1.067)	(1.228)	(1.350)	(1.464)	(1.554)	(1.656)
Extreme Poverty	-1.615	-2.543**	-3.233***	-3.502***	-4.313***	-4.078***	-3.065***
	(1.339)	(1.089)	(1.136)	(1.209)	(1.403)	(1.122)	(1.101)
Gini Index	0.023*	0.014	0.017	0.008	-0.002	-0.005	-0.000
	(0.013)	(0.015)	(0.015)	(0.015)	(0.016)	(0.018)	(0.017)
Fixed Effects	Year, Mun	Year, Mun	Year, Mun	Year, Mun	Year, Mun	Year, Mun	Year, Mun
Observations	919	633	560	493	439	406	386
Municipalities	346	332	323	318	308	302	299
Clusters	57	55	53	47	46	43	41

Notes: Standard errors clustered at state-year level in parenthesis. *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table 13: Average Treatment Effect on the Treated (ATT), with state and region specific trends

	(1)	(2)	(3)
HDI	0.011***	0.011***	0.010***
	(0.004)	(0.003)	(0.003)
Years of Schooling	-0.099	-0.096	-0.118
	(0.106)	(0.103)	(0.092)
Income per capita	5.703	5.434	6.674
	(12.214)	(11.187)	(10.469)
Piped Water	1.631	1.698	1.288
	(2.209)	(1.729)	(1.499)
Garbage Collection	0.576	0.772	0.599
	(2.004)	(1.713)	(1.660)
Access to Electricity	0.482	0.546	0.378
	(1.239)	(1.129)	(1.070)
Illiteracy 11-14 years	0.839	0.837*	1.005**
	(0.528)	(0.464)	(0.444)
Illiteracy 15+ years	0.080	0.063	0.135
	(0.274)	(0.269)	(0.241)
Infant Mortality	-0.850	-0.892	-0.814
	(0.872)	(0.724)	(0.720)
Extreme Poverty	-0.015	-0.060	-0.083
	(0.722)	(0.582)	(0.574)
Gini Index	0.010**	0.010**	0.010**
	(0.005)	(0.005)	(0.005)
Fixed Effects	Year,Mun	Year,Mun	Year,Mun
State×Year	No	Yes	Yes
Region×Year	No	No	Yes
Observations	1,543	1,543	1,543
Municipalities	587	587	587
Clusters	113	113	113

Notes: Standard errors clustered at state-year level in parenthesis. *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table 14: Average Treatment Effect on the Treated (ATT), with state and region specific trends, excluding municipalities where damns and engine rooms were built

	(1)	(2)	(3)
HDI	0.009***	0.009***	0.009***
	(0.003)	(0.003)	(0.003)
Years of Schooling	-0.184	-0.180	-0.198**
	(0.112)	(0.108)	(0.097)
Income per capita	8.551	8.244	9.091
	(12.590)	(11.426)	(10.886)
Piped Water	1.247	1.317	0.973
	(2.202)	(1.690)	(1.492)
Garbage Collection	-0.096	0.051	-0.078
	(2.102)	(1.822)	(1.803)
Access to Electricity	-0.179	-0.111	-0.248
	(1.227)	(1.163)	(1.114)
Illiteracy 11-14 years	0.799	0.804*	0.952**
	(0.554)	(0.478)	(0.476)
Illiteracy 15+ years	0.101	0.083	0.136
	(0.321)	(0.303)	(0.281)
Infant Mortality	-0.577	-0.633	-0.555
	(0.923)	(0.762)	(0.768)
Extreme Poverty	-0.072	-0.117	-0.118
	(0.801)	(0.701)	(0.696)
Gini Index	0.009*	0.009*	0.009*
	(0.005)	(0.005)	(0.005)
Fixed Effects	Year,Mun	Year,Mun	Year,Mun
State×Year	No	Yes	Yes
Region×Year	No	No	Yes
Observations	1,157	1,157	1,157
Municipalities	441	441	441
Clusters	103	103	103

Notes: This sample excludes municipalities where dams and engine rooms where built. Standard errors clustered at state-year level in parenthesis. *** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table 15: Balancing of covariates, excluding municipalities where dams and engine rooms were built

	Baseline	Common Support	Common Support & Propensity Score
Elevation	0.146***	0.023	0.038
	(0.018)	(0.023)	(0.024)
Water Accumulation	1.727***	0.869***	0.001
	(0.039)	(0.053)	(0.049)
Transmission Lines	0.226***	0.126***	0.008
	(0.011)	(0.016)	(0.016)
Intergovernmental Transfers	-3.878***	-1.594***	0.045
	(0.349)	(0.449)	(0.471)
GDP per capita	2.304***	0.759***	0.014
	(0.172)	(0.250)	(0.262)
Population	0.131***	0.118***	0.039
	(0.026)	(0.036)	(0.038)
Area	0.175***	0.033	0.009
	(0.030)	(0.040)	(0.042)
Dependency Ratio	-7.666***	-1.779***	0.171
	(0.371)	(0.397)	(0.414)
Urban Population	10.994***	5.127***	-0.316
	(0.544)	(0.699)	(0.717)

Notes: Robust standard errors in parenthesis. *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 16: Balancing of covariates, by decile, excluding municipalities where dams and engine rooms were built

	Elevation	Water Accum.	Intergov. Transfers	GDP per Cap.	Population	Area	Dependency Ratio	Urban Population
1st Decile	0.430***	0.021	0.666	-0.114***	-0.001	0.084	0.290	-0.722
	(0.095)	(0.066)	(0.962)	(0.036)	(0.026)	(0.058)	(0.187)	(0.782)
2nd Decile	0.014	0.002	-0.245	-0.008	0.009	0.005	-0.179*	-0.014
	(0.010)	(0.018)	(0.234)	(0.019)	(0.011)	(0.013)	(0.094)	(0.368)
3rd Decile	-0.012	-0.019	-0.100	0.021	-0.002	0.007	-0.109	0.020
	(0.007)	(0.017)	(0.157)	(0.020)	(0.009)	(0.009)	(0.089)	(0.320)
4th Decile	-0.007	0.000	-0.030	0.001	-0.002	0.007	-0.064	0.105
	(0.006)	(0.017)	(0.124)	(0.022)	(0.009)	(0.008)	(0.086)	(0.254)
5th Decile	-0.006	-0.057***	0.047	0.008	0.016	-0.007	0.059	0.018
	(0.006)	(0.016)	(0.103)	(0.024)	(0.011)	(0.008)	(0.090)	(0.243)
6th Decile	0.001	-0.010	0.257***	-0.001	-0.002	0.009	0.095	0.001
	(0.005)	(0.017)	(0.099)	(0.022)	(0.008)	(0.011)	(0.106)	(0.219)
7th Decile	-0.008	-0.001	0.049	-0.001	-0.005	0.024**	0.258	0.305
	(0.006)	(0.016)	(0.100)	(0.023)	(0.009)	(0.010)	(0.180)	(0.224)
8th Decile	-0.004	0.002	-0.369***	0.004	0.007	-0.007	-0.478	0.328
	(0.006)	(0.018)	(0.132)	(0.030)	(0.011)	(0.014)	(0.294)	(0.201)
9th Decile	-0.004	0.001	0.040	0.014	0.025	-0.001	1.281**	-0.164
	(0.009)	(0.022)	(0.096)	(0.057)	(0.016)	(0.023)	(0.577)	(0.203)
10th Decile	0.026	-0.425***	-0.372	-0.990	0.085	-0.230***	-0.282	0.014
	(0.028)	(0.068)	(0.412)	(1.583)	(0.069)	(0.082)	(1.399)	(0.254)

Notes: Robust standard errors in parenthesis. *** significant at the 1% level. ** significant at the 5% level. * significant at the 10% level.